INTRODUCTORY LECTURE

TO THE

COURSE OF

COMPARATIVE ANATOMY,

DELIVERED AT

The Royal College of Surgeons of England,

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INTRODUCTORY LECTURE.

Mr. President and Gentlemen,—I am sure that the feeling that is uppermost in the minds of all here assembled is regret at the cause which has placed me before you on this occasion. Of the able and distinguished men who have filled this chair since it was instituted in the commencement of the century, not one was more able or more distinguished than its last occupant, either as an original investigator in the branch of science he adorns or as a facile expositor of its truths. The loss of Professor Huxley's services to this Institution is indeed to be deplored. The only consolation is that what is loss to us may be gain to others. He but leaves us that he may concentrate his time, his energies, his genius elsewhere.

Feeling as deeply as I do the responsibilities of the Hunterian professorship, and my incapacity to discharge its duties in a manner worthy of its dignity and importance, I should certainly have

declined to undertake it when it was proposed to me, if it had not been for the thought that these lectures are emphatically *museum* lectures, intended to illustrate and explain the treasures of our noble collection, and that from these has been derived almost all I know of the great subject that I am now called upon to teach. I shall therefore, perhaps more than another might, speak to you directly from those specimens. I am, as it were, *their* mouthpiece. Having lovingly dwelt and worked among them so long, I felt that I could hardly refuse to tell you something of what they are always telling me—to endeavour to put their silent eloquence in some sort of articulate language. If I can only succeed in awakening an interest in them, and lead any one who may chance to come into this theatre to continue his studies in the museum, where he will meet with far greater return for his time and his attention than here, the delivery of these lectures will not have been altogether in vain.

I have just mentioned the museum. It is impossible to do so without recalling the circumstance that this day is the one on which we have been accustomed to celebrate the anniversary of the birth of the illustrious founder of that collection.

February 14 is a red-letter day in our calendar. On this day a series of eloquent tributes have been paid to the merits of that great man by successive Hunterian orators. I will not delay

the commencement of the proper subject of this course by endeavouring to speak of Hunter either as a philosopher, a physiologist, or a surgeon, but I cannot refrain from making a passing allusion to his work in relation to the special subject of these lectures—*Comparative Anatomy*. And even this would have been superfluous, after the able analysis of Hunter's work in this branch of science contributed by Professor Owen to the fourth volume of Palmer's edition of his collected writings, if it had not been for the vast elucidation of the nature and amount of that work by the subsequent publication of these two closely printed octavo volumes of "Essays and Observations on Natural History, Anatomy, Physiology, Psychology, and Geology, by John Hunter, being his Posthumous Papers on those Subjects," arranged and revised by Professor Owen (1861).

Hunter's reputation has suffered grievously from the extreme difficulty he always met with in giving adequate expression to his ideas. Mr. Clift, who acted for a time as his amanuensis, has told us how he has often "written the same page for him at least half a dozen times over, with corrections and transpositions almost without end;" and those who are familiar with his writings must own that, after all this labour, the result was often far from satisfactory. Hunter was himself painfully aware of the deficiency. This it is which has detracted much from the

estimate in which many of his published works are held, and it is this which was, I suspect, the reason why his published writings bear so small a proportion to the vast mass of rough manuscript which he left behind him, especially on the subjects with which we are at present engaged.

The perusal of the work just mentioned will show how, while occupied with a large and anxious practice—in itself labour enough for ordinary men—while cultivating with what I might describe as a passionate energy the sciences of physiology and pathology, while collecting and arranging a museum such as never has been formed before or since by a single individual, he had also carefully recorded a series of dissections of different species of animals which, as his learned editor justly says, if "published *seriatim*, would not only have vied with the labours of Daubenton as recorded in the 'Histoire Naturelle' of Buffon, or with the 'Comparative Dissections' of Vicq d'Azyr, which are inserted in the early volumes of the *Encyclopédie Méthodique* and in the *Mémoires de l'Académie Royale de France*, but they would have exceeded them both together.* In fact, they would have established Hunter's fame as by far the greatest contributor to the knowledge of animal structure of his time, and, what is more important, would have aided most materially in the advancement and diffusion of that knowledge.

* Palmer's edit. vol. iv. p. vi.

The work as now published contains notes of dissections, more or less complete, of no less than 129 species of mammalia, 80 species of birds, 20 species of reptiles, 9 of amphibia, and 19 of fishes, besides numerous invertebrated animals of various classes; and these appear to be by no means the entire series of dissections left by Hunter.

The fate of the original manuscripts forms a sad page in the history of our science, and, I must also add, in the history of a man whom our Profession, and this College especially, might otherwise have regarded with feelings of respect and gratitude. Exactly thirty years after Hunter's death, his brother-in-law and executor, Sir Everard Home, who had retained possession of them when the collection was transferred to the College, committed them all to the flames. The use that he had made of them in composing the lectures which he delivered from this chair, though often previously surmised, has become but too obvious since the publication of this work.*

* An account of this transaction, with some important remarks upon it by Mr. Clift, is appended to Professor Owen's edition of Hunter's "Essays and Observations" referred to above. The late Sir Benjamin C. Brodie, in his autobiography, prefixed to Mr. Charles Hawkins' edition of his works (1865, vol. i. p. 102), speaks of it in the following words:—"Some years before his [Sir Everard Home's] death, he got great discredit from having destroyed a considerable portion of John Hunter's manuscripts which had come into his possession as one of Hunter's executors. This act was equally unjustifiable

Happily, Hunter's devoted assistant and friend, the assiduous and excellent custodian of his collection for nearly fifty years, William Clift, had occupied himself in the interval between Hunter's death and the removal of the manuscripts to Sir Everard Home's house, in making copious extracts from these precious documents. The publication of these extracts, nearly seventy years after their author had passed away, however meritorious a labour on the part of their editor, can unfortunately do little to regain for Hunter that place among the leaders of science which his own untimely death, and the negligence, or worse, of

and foolish. It was unjustifiable, because the manuscripts should have been considered as belonging to the museum, which Parliament had purchased; and it was foolish, because it has led to the notion that he had made use of John Hunter's observations for his own purposes, much more than was really the case. I had frequent opportunities of seeing these papers during nine or ten years, in which I was accustomed, more or less, conjointly with Mr. Clift, to assist him in his dissections. They consisted of rough notes on the anatomy of animals, which must have been useful to Hunter himself, or which would, I doubt not, have afforded help to Mr. Owen in completing the catalogue of the museum; but they were not such as could be used with much advantage by another person. In pursuing his own investigations, Home sometimes referred to these; but I must say that, while I was connected with him, I never knew an instance in which he did not scrupulously acknowledge whatever he took from them, or do justice to his illustrious predecessor Unhappily, he was led afterwards to deviate from this right course; and in his later publications I recognise some things which he has given as the result of his own observation, though they were really taken from Hunter's notes and

his executor, had deprived him. Highly original and valuable as are the observations contained in these volumes, they have by this time been nearly all anticipated by others. A crowd of workers in the same field, pressing in from all directions, have covered the ground which ought to have been occupied by the figure of Hunter, and from which no tardy recognition of his merits can ever dispossess them. We shall still continue to look to Cuvier and to Meckel as the main sources of our modern knowledge of comparative anatomy, and not to Hunter. Let us though, never forget that our illustrious countryman had,

drawings. One of these is a paper on the progressive motion of animals, and another a series of engravings, representing the convolutions of the intestinal canal, and neither of them of much scientific value."—See also a note on the same subject in the obituary notice of Sir B. Brodie in the *Lancet*, October 25th, 1862.

It is very possible that the extent to which Home made use of these manuscripts had been much exaggerated; but Sir Benjamin certainly did not fully appreciate either their value or the amount of Home's plagiarism. It is not in his latest writings, but in the first volume of his lectures on Comparative Anatomy, published in 1814, that these chiefly occur, as will be seen by the editorial notes appended by Professor Owen to Hunter's Essays. The only justification which can be set up for Home is, that the observations quoted without acknowledgment were really made by him, when acting as Hunter's pupil or assistant, and had become incorporated in the papers of his master; but nothing of the kind appears ever to have been alleged, and this, of course, would be no excuse for first retaining possession of, and subsequently destroying nearly the whole of the manuscripts.

before their time, collected materials for a work which needed but the finishing touches to have made it one of the greatest, most durable, and valuable contributions ever made by any one man to the advancement of the science of comparative anatomy.

The present course of lectures, as just said, are intimately associated with the museum; their annual delivery was one of the conditions on which the care of the Hunterian collection was entrusted by the Government to our College, and it was expressly stipulated that they should be "illustrated by the preparations." I am bound, therefore, in my choice of subjects to consider what the museum teaches, and what it does not teach. The museum teaches one subject, and, primarily, only one subject—namely, the variations in the form and relations of the different parts or organs of which the bodies of various animals are composed. In brief, it teaches *Animal Morphology*, which is nearly synonymous with *Comparative Anatomy*.

Now, *morphology*, I need hardly say, is not *physiology*, though it may be one of the foundations on which that complex science is based. But, contrary as it may seem to some of the most cherished notions of the founder of the collection, an anatomical museum can scarcely do more in teaching physiology than a collection of minerals

can in teaching chemistry. The physiologist should certainly be well versed in the nature of the materials and organs by which the functions of which he treats are carried on—having, indeed, need of all the help he can obtain from all quarters in the solution of the difficult problems which come before him, but his science is, in the main, experimental. Whether it be from the inherent difficulties that attend such inductions, or from the hasty and illogical way in which they have often been made, hitherto a large proportion of the attempts to solve physiological inquiries by an appeal to morphology alone have ended in failure, often of a mortifying character. We still have to confess our ignorance of the purpose and application of innumerable most obvious and striking modifications of structure, which no amount of reasoning or guessing, without actual observation on the living organism, seems able to remove. I am speaking of physiology in the sense in which the term is ordinarily restricted. There is a more general and higher physiology to which I shall refer presently, to elucidate which morphology is one of the most essential aids.

When morphology was first cultivated with anything like scientific precision, views since called "teleological" exclusively prevailed. Every animal was looked upon as an isolated machine; every part of that animal was supposed to have been formed expressly for carrying on the

economy of that particular species or individual in the most efficient manner, without any reference to other species or individuals. If anything further was looked for in anatomising an animal beyond the mere gratification of curiosity or love of knowledge for its own sake, it was *direct adaptation to purpose.*

Many, indeed, are the curious speculations indulged in by anatomists of this school, impelled as they appear always to have been, to find an immediate use for every modification of form. In numerous instances these were mere conjectures, which an enlarged knowledge of the habits and economy of the animal treated of, or of kindred species, showed to be fallacious.

As time went on, however, and men began to obtain a deeper insight into these subjects, resemblances either between the whole structure or between particular parts of different animals, which could not be explained on the utilitarian principle, became strikingly obvious. Moreover, such things as rudimentary and functionless organs in one animal representing developed or functional organs in another, became known in the further prosecution of morphology. Then the idea gradually dawned that there was some "secret bond" which linked creature to creature, and which permitted deviation only to a certain point from a certain given pattern of construction.

This was the doctrine of *type* or *common plan.*

A "type" or a "common plan" for each natural group of the animal kingdom was supposed to have influenced, or to have been held in view at, the creation of every different species composing that group, the deviations from this type being related to the special exigencies of the particular species.

This was a great step in advance when regarded as a mere exposition of the facts of morphology, and when the idea was not carried out by fanciful imaginations beyond the point warranted by these facts. Upon this view many anatomists of great eminence seemed to rest. But still it explained nothing, accounted for nothing. It gave not even a shadow of a reason for the resemblances amid diversity found everywhere. It only asserted that the Creator had imposed certain apparently quite arbitrary restrictions to His power; but, beyond this almost paradoxical assertion, it gave no clue to elucidate anything like a theory of creation.

In the meanwhile, however, the great results arrived at in other branches of science, the increasing accumulation of facts from various sources, all tending to show that the orderly and harmonious working of the whole universe was due to constantly acting causes or laws, acting now, having acted for immeasurable time past, and, as far as we can see, about to act for immeasurable time to come; the great work of the astronomers and the geologists, leading to "the

general conception of some *great principle of orderly evolution*, according to which the present as well as past systems of existence have been produced out of preceding orders of things,"* could not pass unnoticed by the biologists.

First with a faint and uncertain sound, but in later times more boldly and confidently, an hypothesis has been propounded, which does profess to account for some, at least, of the facts of animal creation, and to afford a guide to the solution of the problem, if not of the original beginnings, at least of the present diversities, as well as resemblances, among the animal and vegetable life on the globe.

This theory has for its basis that the secret bond of union is not "conformity to type," is no ideal to which the operations of Creation were limited, but is one of actual consanguinity, or, as more concisely expressed, "genetic affinity."

The fundamental part of the theory is, in other words, that, as individuals are known to come into being by a process of generation acting according to fixed and certain laws, the same to-day and yesterday and to-morrow, in like manner have races, varieties, species, and other larger groups of animals and plants come into being—that the species existing at any one period on the earth's surface are, in fact, the direct

* Baden Powell, "Unity of Worlds and of Nature," 2nd edition, 1856, p. 448.

descendants, modified according to definite laws, of the creatures inhabiting the earth in previous periods.

The theory of orderly evolution already applied to most of the phenomena of the physical world has thus been also applied to organic nature.

This hypothesis, originally promulgated in an extremely crude form by De Maillet, Erasmus Darwin, and Lamarck, and more recently advocated in an equally crude form by the author of the "Vestiges of Creation," for a long time found little favour with English naturalists of eminence. Indeed, so late as 1856, Baden Powell, in his masterly essay "On the Philosophy of Creation," arguing with great force "that one grand over-ruling principle, the universality of law, order, and continuity, presiding as powerfully over the earlier stages of creation as during its continuance at the present moment, applies equally to organic as to inorganic existence," can find little support from the writings of any who had made the diversities of organic life their special study. It was, in fact, in opposition to the views then held by most naturalists, and chiefly from a profound and philosophical analysis of the laws which govern the physical universe, that, reasoning from the general to the special, he came to the conclusion that "at least, *as a philosophical conjecture*, the idea of transmutation of species under adequate changes of condition, and in incalculably long

periods of time, seems supported by fair analogy and probability."

Very shortly after, however, these views received an immense stimulus by the working out of a necessary complement to the main theory, one which was absolutely essential to its general reception—namely, a suggestion of a possible and intelligible *modus operandi*—almost simultaneously by the studies of Wallace amid the exuberant displays of nature in the unexplored forests of the Malayan archipelago, and by the patient accumulation and careful and candid examination of fact upon fact, drawn from every branch of biological science, and all converging on the theory of "*the origin of species by means of natural selection*," by the great naturalist* whose name is now so thoroughly identified with the entire transmutation hypothesis, that already in the German booksellers' catalogues "Darwinismus" is made a prominent subdivision in their classification of scientific literature; and in this country "Darwinianism" is the term popularly, if not quite correctly, applied to the general doctrine of "organic evolution."

In reference to the effect which the publication of these researches has produced, the learned and judicious President of the Linnæan Society, Mr. Bentham, in his last anniversary address, says

* Charles Darwin, grandson of Erasmas Darwin, mentioned on the last page.

that "the investigation of the origin, development, and life history of species or races" has been termed *the great problem of the day;* and the impulse they have given to the study of biology in general, and to our special branch in particular, is not exaggerated by the eminent German anatomist Gegenbaur in stating, in a work just published, that "*the theory of descent will begin a new period in the history of comparative anatomy.*"

On the other hand, it happened rather unfortunately that popular views of natural theology, probably through the great influence of a famous work published during what I have described as the first or "utility" stage of morphology, have attached themselves more or less to the ideas of that period, and well-meaning but mistaken persons regard with aversion and alarm the modern theories which are, to a certain extent, subversive of those ideas. I have no hesitation in saying *mistaken*, for it is perfectly evident that all arguments as to "the power, wisdom, and goodness of the Creator," derived from an animal structure not miraculously created, but produced by the ordinary laws of generation, as all known animal structures are, must be entirely and equally valid, whether the laws producing that structure have been operating for a shorter or longer period.

As Professor Asa Gray has well put it, "If the argument from structure to design is convincing when drawn from a particular animal, say a

Newfoundland dog, and is not weakened by the knowledge that this dog came from similar parents, would it be weakened if, in tracing his genealogy, it were ascertained that he was a remote descendant of the mastiff or some other breed, or that both these and other breeds came (as is suspected) from some wolf? If not, how is the argument for design in the structure of our particular dog affected by the supposition that his wolfish progenitor came from a post-tertiary wolf, perhaps less unlike an existing one than the dog in question is to some other of the numerous existing races of dogs, and that this post-tertiary came from an equally or more different tertiary wolf? And if the argument from structure to design is not invalidated by our present knowledge that our individual dog was developed from a single organic cell, how is it invalidated by the supposition of an analogous natural descent, through a long line of connected forms, from a cell, or from some simple animal existing ages before there were any dogs?"*

Those who recognise that the ebb and flow of the tides, the thunderstorms, the rains, and frosts are beneficent in their effects, although the result not of direct miraculous interference, but of unchanging cosmical laws, have not the right to

* "Natural Selection not inconsistent with Natural Theology: a Free Examination of Darwin's Treatise on the Origin of Species." Boston, 1861.

accuse of want of reverence the men who affirm that the wonderfully complex and beautifully adjusted contrivances of animal structure may also have been brought about through the intervention of agencies of similar nature.

This opposition, moreover, may have done harm in another manner, by evoking the natural tendency that exists in many earnest and intensely truth-loving minds to recoil against any display of unphilosophical dogmatism, and any appeal to passions and prejudices, where reason alone ought to have sway, and may thus have led in some cases to a too warm and partial adoption of theories condemned on such grounds.

The rising school of biologists are destined to live in troublous times. There can be no doubt that many of the questions now opening consequent upon the rapid and widespread acceptance of the evolution theory, will give more disquiet to a large class of persons otherwise not indisposed to welcome the advances of scientific discovery, than was ever given by the promulgation of the astronomical revelations of the sixteenth century, or the more recent establishment of the high antiquity of the earth, and are likely to lead to equally active reprisals.

The astronomers and geologists have in their turn had to confront the storm—they and their sciences have survived and triumphed; and, on the other hand, the faith and morality of the

world have not suffered. Now the biologists are standing in the breach. As a part of or necessary sequel to the great doctrine of organic evolution, the question of the origin and position of Man will inevitably obtrude itself. In the coming discussion on this subject, all who take part in it, or alarm themselves about it, would do well calmly to consider this point. It is one similar to that just mentioned with regard to the so-called "natural theology."

Whatever man's place may be either *in* or *out* of nature, whatever hopes, or fears, or feelings about himself or his race he may have, we all of us admit that these are quite uninfluenced by our knowledge of the fact that each individual man comes into the world by the ordinary processes of generation, according to the same laws which apply to the development of all organic beings whatever, that every part of him which can come under the scrutiny of the anatomist or naturalist has been evolved according to these regular laws from a simple minute ovum, indistinguishable to our senses from that of any of the inferior animals. If this be so—if man is what he is, notwithstanding the corporeal mode of origin of the individual man, so he will assuredly be neither less nor more than man, whatever may be shown regarding the corporeal origin of the whole race, whether this was from the dust of the earth, or by the modification of some pre-existing

animal form. This, I conceive, is the ground on which those who would maintain unimpaired the spiritual and moral dignity of the human race may safely stand, resting assured that scientific research, having for its sole end the discovery of Truth, can never lead to aught that is opposed to the best and highest interests of mankind.*

In 1861 one of the most distinguished of American naturalists wrote:—"Those, if any there be, who regard the derivative hypothesis as satisfactorily proved must have loose notions as to what proof is. Those who imagine it can be easily refuted and cast aside must, we think, have imperfect or very prejudiced conceptions of the facts concerned and of the questions at issue." The experience of nine years leaves the case precisely, in my judgment, as thus stated by Professor Asa Gray. Anything which assists to throw light upon it, to lead us nearer either to its acceptance or rejection, is of primary importance to the biologist. We must not refuse to take it into earnest consideration.

I think, therefore, that it may be worth while to devote a few words to two objections which are frequently urged, not indeed against this theory, but against the prominent place I would assign to its consideration by all who would enter deeply into the philosophy of biology. Both of

* For the bearings of the Evolution theory of Creation on this subject, see Baden Powell, *op. cit.*

these objections are founded upon misconceptions as to the real nature of the present aspect of the theory of development.

The first, taking its leading idea from the title of Mr. Darwin's book, is that it is unprofitable work to trouble ourselves about the *origin* of species, or, indeed, of anything else. These events of the very remote past, it is said, are quite beyond our ken; we really can never know anything certain about them, and had far better occupy ourselves with things of the present—things which we can really hope to know much about.

The other, akin to this, has lately been put forth on high authority. Let the "derivative hypothesis" be granted, the studies of the zoologist are not thereby in any way affected. "For all intents and purposes of the descriptive and recording naturalist," it is said, "species are constant; they will last our time. When the existing binomial units of botanical and zoological specific lists cease to show their present distinctive characters, the *homo sapiens* of Linnæus will have merged into another, probably a higher specific form."*

As I have said, both these objections are founded on an entire misconception of the main question at issue. If the developmental theory as held by Darwin, Wallace, and their followers is true, the origin of species is as much a thing

* Owen on the "Aye-Aye." *Trans. Zool. Soc.*, vol. v. p. 92.

of the present as of the past. It is an essential part of the theory that the laws which have produced the diversity of organic forms in the world are those by which the world not only has been, but *is* governed. They are as constant, ever-acting, and unchanging as the laws which direct the movements of the clouds or cause the torrents to flow down from the mountain side. There is no proof whatever that the laws of variation and natural selection, if such be the laws which lead to the introduction of new forms and the extinction of old ones, were ever more potent than they are at present. A large class of the arguments by which the theory is supported is derived from observation upon the present phenomena of life. According to the hypothesis, transitional forms and incipient species are to be met with everywhere around us. It is this, in fact, which has given rise to the difficulty zoologists and botanists always meet with in defining the limits of the various so-called species composing so many groups both of animals and plants. Until it is settled whether there is an insensible blending in the conditions expressed by the terms "variety," "race," "subspecies," "species," or whether the old idea of the immutability of species is to be maintained, zoology can hardly be said to have a philosophical basis.

The second objection just named appears to suppose a simultaneous march of all organic

beings from form to form—a transmutation *en masse* of the whole—so that to any one among them they would all appear stationary. Such a view is utterly opposed to the greater number of zoological and palæontological facts, and to all the necessities of the Darwinian hypothesis. One species, either with little inherent capacity for variation, or so circumstanced that such variation as may have occurred in different individuals has never been accumulated by selection, may remain without alteration for ages, while other species differently constituted or circumstanced may have undergone vast transmutations during the same period. In fact, the derivative hypothesis is not a theory of things long ago; it is not a curious speculation into the beginnings of the present condition of things. It may be appealed to every day in the solution of constantly occurring problems in morphology.

I will give an example from the subject of our present course. There is in Australia an animal, the koala (*Phascolarctos cinereus*), rather larger than a cat, but having more the aspect of a small bear. It is covered with a soft woolly fur, and lives entirely among the boughs of trees, on the leaves of which it feeds. Its tail is a mere rudiment. Its feet are admirably adapted for grasping the branches on which it climbs. The hind feet especially are remarkably modified for this end, being very broad, and having a strong prehensile

or opposable inner toe or hallux, like a thumb.

Fig. 1.

Fig. 1.—The Koala (*Phascolarctos cinereus*). From Gould's "Mammals of Australia."

The skeleton of that foot is very singular (see Fig. 2). The hallux is stout, and placed nearly

Fig 2.

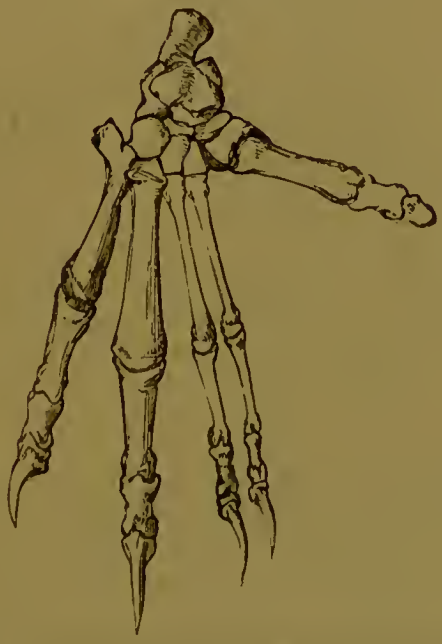

Fig. 2.—Skeleton of the hind foot of the Koala (*Phascolarctos cinereus.*)

at a right angle with the other toes. The next two digits are rather slender, placed close together, and in the living animal are united almost to the claws in a common integument. The two outer toes are free and much stouter than the others, the fourth especially so.

Now, a naturalist of the last century would have looked upon this foot simply as a piece of machinery beautifully adapted for the particular work it had to perform in the animal's life. He would have pointed to the broad palm, the opposable great toe, and would doubtless, if a worthy representative of his school, have developed some ingenious theory to show that the

FIG. 3.

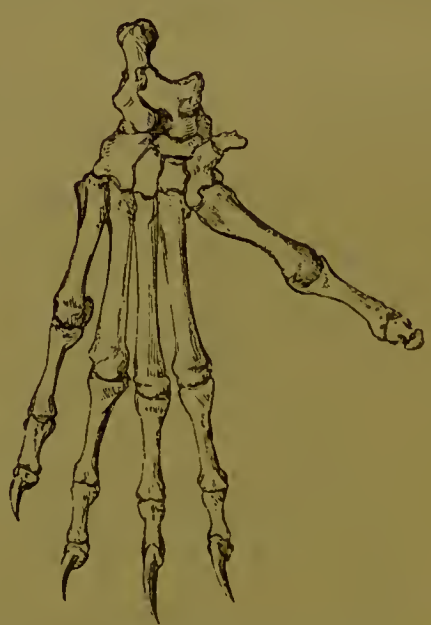

FIG. 3.—Skeleton of the hind foot of the Virginian Opossum (*Didelphys Virginiana*).

smaller size of the second and third toes, and that being wrapped up to their ends in skin, added to

the efficiency of the foot as a climbing organ, though indeed he might be at a loss to say why the American opossums, of precisely similar habits, and with similarly opposable great toes, have all their other four toes free and equal.

The philosophical naturalist of the second of the types that I have sketched out, in investigating this structure, will institute a careful comparison between the foot of the koala and that of other animals belonging to the same group.

Among the marsupial animals of Australia, few are so well known as the kangaroo. Like the koala, it is a vegetable feeder, and these two forms have some dental and other structural characters in common. But the mode of progression, and the limbs which effect this, offer the greatest possible contrast.

In the kangaroo, the hind legs are disproportionately large. The foot especially is long and narrow, and at first sight appears composed of a single large toe. Its motion along the ground consists of a series of leaps or hops effected entirely by these powerful hind limbs. On looking more closely at the foot (see Fig. 4), it will be seen that the first digit or hallux is entirely wanting, but the other four toes are present with their complete number of phalanges. The fourth is immensely developed, the fifth moderately so, but the second and third are reduced to the most

slender rudiments, and are so united in life by a common integument, that they look like a single toe with a double claw.

Fig. 4.

Fig. 4.—Skeleton of the hind foot of the kangaroo (*Macropus major*).

Now, here are two feet as unlike as possible in their functions—the one formed for rapidly hopping along over the arid plains, the other for slowly and securely climbing among the boughs of the forests—yet presenting a deep-seated resemblance in a character not found in the feet of any other known mammal except the immediate allies of these two.

We may call this "conformity to type" without getting much nearer to an explanation of the phenomenon. Perhaps it is safest to rest at this stage.

But is it not powerfully suggestive—I will not say more, for, of course, by itself it cannot be considered as a proof—of true relationship, of inheritance from a common ancestor? We can easily see that in some manner the great preponderance of the one toe in the kangaroo, and the reduction of the others, would be advantageous in its mode of progression, reducing the foot to a narrow spring-board as it were; and we can see

that this attenuation of the second and third toes, after the first had quite disappeared, might be a stage in the direction of their total disappearance. But while this was taking place, let us suppose that one branch of the family took to climbing trees (I must not enter fully into all that might be supposed on this subject, as I am merely introducing this as an example of the mode in which morphological problems are illustrated by these general views), and the variations in a particular direction, tending towards better climbing, were preserved and accumulated on the Darwinian hypothesis, the hallux, (a common part in all mammals, only in abeyance when not wanted,) was redeveloped in a form best adapted for its actual purposes, the other digits gradually resuming their normal condition; these two, however, still preserving strong reminiscences of the ancestral state.*

* In New Guinea there are actually two species of kangaroos, constituting the genus *Dendrolagus*, which habitually reside in trees; not climbing among the smaller boughs, as is the manner of the koalas and opossums, but sitting on the larger horizontal branches. Their feet present a marked deviation from those of the common kangaroos of the plains, the deviation being *in the direction* of the feet of the koala. They are shorter and broader; the lateral toes are relatively more developed, and are on the same plane with the fourth or large toe, and although they have no hallux or first toe, the bone of the tarsus (internal cuneiform), which usually supports that digit, is of relatively larger size.

The phalangers, a family of climbing vegetable-feeding

It is certainly significant that all the Australian vegetable-feeding climbing opossums have feet constructed on this type, one that is similar to that of the kangaroos of the same country, while the American opossums, further removed geographically, and therefore, in all probability, in actual relationship, with feet functionally the same, show no trace of this deep-seated structural peculiarity—a peculiarity most important for the consideration of the philosophical anatomist, as it evidently depends on some more far-reaching cause than mere adaptation to purpose.

In considering a little more fully the application of such views to the study of morphology, I must say a few words on *classification*. It was felt at the very outset of the study of natural history that without some system of classification the subject was little more than a hopeless chaotic confusion. The first instinct of a zoologist is to arrange in some sort of order the multitudinous objects with which he has to deal. In the beginning of science, with little sound knowledge, such classifications were often mere arbitrary arrange-

marsupials, form another link in the structure of their feet between the kangaroo and the koala, though most nearly resembling the latter. On the other hand, in a very remarkable little ground-dwelling animal, the *Chæropus*, the kangaroo type of foot is modified in the opposite direction, all the digits except the fourth being reduced to excessively rudimentary proportions.

ments, founded on some easily accessible peculiarities, and forming nothing more than a means by which the distinction between certain objects and certain other objects could be grasped by the mind of the founder and communicated to others. Classifications based on such characters became so general, and were often so carelessly and ignorantly put together, that they threatened to bring the whole system into disrepute; whereas I have little hesitation in saying that, in reality, *classification* is one of the, if not the, most important aims and ends of the study of morphology. It is the best contribution which we can make towards the solution of the great biological problem, for a true classification, viewed by the light of the derivative hypothesis, is nothing more or less than an expression of the actual amount of affinity between different objects. An order, a family, or a genus is no longer a group of animals linked together by some arbitrarily selected characters, but a group that are supposed to have descended from a common ancestor, and have become, by whatever process, gradually differentiated from other groups of animals.*

* In examining into the validity of the derivative hypothesis, much is to be expected from the study of the geographical distribution of animals and plants, both in present and in former times; but such study will be quite in vain unless morphologists have first determined correctly the affinities of the animals and plants treated of. It is obvious that all

As in such groups, when once established, there can be no crossing with other groups (as in human families, to which they are sometimes compared) all the resemblances which are found between members of different groups must either be characters inherited from the common ancestor, perhaps lost through many generations and reappearing at a subsequent period by the process of "reversion," or they must be characters having merely *analogical* resemblances—*i.e.*, characters developed by variation, but which, by similarity of conditions of existence or other causes, have become similar to each other.

To discriminate between these two classes of characters—namely, those that are essential and fundamental, or, in the language of one school, are dependent on conformity to type, or, according to the derivative hypothesis, are inherited from remote ancestors, and those that are modifications to suit the conditions of existence to which the animal is exposed, or, in other words, adaptive characters—is a constantly recurring problem to the systematic morphologist; and the difficulties that encompass the solution of this problem are the main causes of the little progress hitherto made towards a general agreement as to classification otherwise than in

inferences from geographical or palæontological research are useless, unless the *classification* on which they are based is at least approximately true to nature.

the main groups. Let me illustrate this point by a single and, as it happens, by no means difficult example.

You have before you the skeletons of two animals very similar in their general or superficial characters—one is the common dog, the other the carnivorous marsupial, the thylacine,

Fig. 5.

Fig. 5.—The Thylacine (*Thylacinus cynocephalus*). From Wolf's "Zoological Sketches."

sometimes called "Tasmanian wolf." These animals, when wild, have very similar habits. You will see that, in the general characters of the skeleton, the structure of the limbs, and the arrangement of the teeth, a remarkable similarity prevails. You might easily conclude that these animals were related. They were, indeed, formerly placed near together by systematists.

Now, if I place by the dog another well-known animal, a sheep, you will see that in many characters of its bones, its feet, its teeth, &c., it

differs far more from the dog than the latter does from the thylacine. Yet zoologists affirm, without the slightest hesitation, that the sheep and the dog are far more nearly related to each other than either is to the thylacine. This assertion is founded on a discrimination of the essential from the adaptive characters. The dog and the sheep belong to one great branch of the mammalian class, although they represent different stocks of that branch. The thylacine belongs to another primary branch, but in that branch it has taken the place, functionally, of the dog in the other; its organs have become adapted to perform the same work.*

It has been asserted, in a recent very able pamphlet by an anonymous writer, "On the Difficulties of the Theory of Natural Selection,"† "that on the theory of 'natural selec-

* The two great branches of the mammalia referred to are—1, the Monodelphous, or placental, comprising the great bulk of the animals of the class; and, 2, the Didelphous, or nonplacental. The latter, which are the animals commonly known as *Marsupialia*, or pouched animals, though varying extremely in the structure of the feet, teeth, &c., also in their food and manner of life, all agree together, and differ from the monodelphia in numerous important characters of the skeleton, brain, heart, and especially of the reproductive organs and processes. The marsupials are, at the present day, entirely confined to Australia, its neighbouring islands, and the American continent, though, in former times, they had a more extensive geographical range.

† Published originally in the "Month," 1869.

tion' it is all but impossible, such are the probabilities against it, that identical structures should have arisen independently. Yet many structures undeniably exist, which, to all appearance, must have so arisen." And the "remark-

Fig. 6.

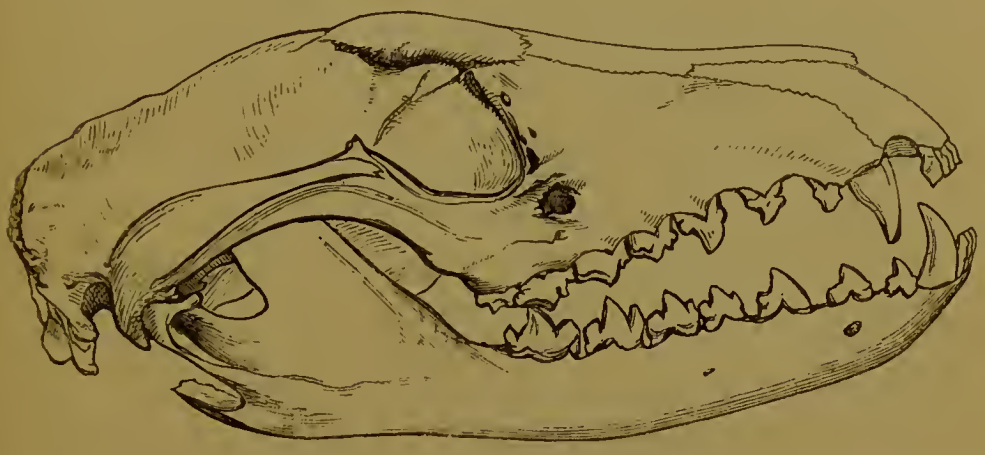

Fig. 6.—Skull of Thylacine.

able identity of structure between certain of the teeth of the large predatory marsupial, the thylacine, or Tasmanian wolf, and those of the com-

Fig. 7.

Fig. 7.—Skull of Shepherd's Dog (*Canis familiaris*).

mon dog," is cited as a case in point. Now, though I am fully willing to admit difficulties in

the theory, quite inexplicable with our present knowledge, as many almost as the author quoted, I cannot see this one in the same light. It appears to me that the probabilities, instead of being against the independent origin of such similar structures (erroneously called *identical*) as those just mentioned, are exceedingly in their favour. I cannot in the least see why a marsupial animal should not be carnivorous and predaceous as well as a placental animal, and if so, why its teeth should not have come to possess the general attributes essential to animals of such habits—namely, large pointed recurved fangs at each of the anterior angles of the jaw; the teeth between them much reduced, so as not to interfere with the piercing and holding action of the canines; the molars more or less scissor-shaped. These are plainly adaptive characters, and whatever process has produced them in the one case is just as likely to have produced them in the other —I might almost say, must have produced them in the other. So far, however, are the teeth of the thylacine and the dog from being essentially "identical," that in numerous non-adaptive and non-functional characters, as the number of the incisors, premolars, and molars, the mode of succession, and the minute structure (all which will be spoken of at a later period of the course), they widely differ, the dog conforming in all these points with the ordinary placental mammals, the

thylacine with the remaining marsupials. Such characters, underlying, in large groups of animals, the various modifications in relation to use, cannot be too diligently sought out as the true landmarks to guide our steps through the intricacies that beset our path in tracing affinities.

Another valuable guiding principle in morphological studies is this. It is a remark of Gegenbaur's, in his valuable work "On the Structure of the *Carpus* and *Tarsus*," but it must have occurred to anyone who has given much thought to these problems. When we wish to discover the distinguishing characters between different organisms, it is necessary to examine them in their most fully developed condition. If, on the other hand, our object is to trace their resemblance, their intimate relationships, we must study them in their early embryonic stages. By these methods we can do much to separate what is secondary or superadded from what is fundamental or essential in the character of an animal. The further back we can carry our researches, the more prominent do the characters, common to the whole group to which the animal belongs, become. The more completely mature the specimen, the more do the special characters of the species or even of the individual predominate.

It is not my province in these lectures to indulge much in speculations. Indeed, as students

of morphology, we are as yet little in a position to do so. I shall not say much more even on the general views to which I have lately referred, for I feel that the acquisition of a sound basis of fact to work with is what is, at present, most needed in comparative anatomy. The difficulties which beset the beginner, and indeed the more advanced student in this subject are very great. As regards the branch to which I propose more particularly to direct your attention, the anatomy of the class Mammalia, which, we might suppose, was better worked out than any other, the information to be found in books is scattered, fragmentary, unequal, and often untrustworthy; even good elementary treatises are wanting, much more anything like an exhaustive work. Moreover, our Museum—superior as I believe it to be to any other of the kind—is, as yet, far from adequate to supply the knowledge frequently sought for in it.

In considering the special subject for these lectures, I have often thought that the greatest permanent benefit would be conferred on our science by collecting together in a systematic form all the available information upon limited groups of the animal kingdom, supplementing, as far as possible, the deficiencies of knowledge by fresh observations, and illustrating the subject by a complete series of preparations. At the present day it is only by working out a definite branch

of limited extent monographically, that any solid advances in detailed knowledge can be attained. Courses of lectures on this principle, especially if they were published so as to reach a wider circle of students than this theatre is apt to contain, might do much to *advance* knowledge; but, on the other hand, they might do less than more elementary lectures giving a general outline of a larger variety of topics, to *diffuse* knowledge; for it is probable that they would be attractive to only a limited number of auditors or of readers.

I think it therefore advisable, in the first course I have the honour of addressing to you, to take a general survey of the structure of the animals of the highest class of organized beings; and I think that this will prove of the greater importance and interest to the audience assembled here, because it may be presumed that all have already either commenced or completed the acquisition of a knowledge of the structural anatomy of one, the most elevated member, of that class. I propose, therefore, to take human anatomy as a point of departure, and, presuming on your acquaintance with its details, shall refer only to some of its general outlines, and shall point out the deviations from and resemblance to the mammalian structure as we know it in man, in descending throughout the series of animals composing the class.

I trust that some further interest may be given

thereby to the daily work of the student of our profession. Human anatomy is too often learned as a mere collection of hard names applied to a complicated network of structures, the form, position, and relation of which have to be got up, probably to be forgotten soon afterwards. It might, however, be made a far more attractive and useful subject if taught by the light of a wider morphology. But I am afraid that very little of our anatomical teaching, either by books or lectures, is of this class at present. If any comparative anatomy is introduced, instead of enlightening and illustrating the subject, it often only adds another load to the already overburdened memory —for instance, after the usual dry, detailed and technical description of the part treated of, a disquisition is added on what is called its "transcendental" anatomy, extremely incomprehensible to most minds, and consisting chiefly in the imposition of a new set of names to parts of which the student has just succeeded in mastering the old ones.

All this ought to be reversed; the essential nature of the part in question, as deduced from comparative anatomy, should be first announced with a glance at its principal modifications, then its special characters as seen in man will be a subject of intelligent interest. I know from experience that after studying and teaching human anatomy for many years on the ordinary methods,

there were many parts the meaning and nature of which I never understood until I began to dissect animals; and increase of knowledge in this direction constantly throws light on apparently unmeaning or incomprehensible parts of human structure.

One word more by way of introduction. I began with the mention of my immediate predecessor in this chair. To him I must once again return. The subject matter of the present course will embrace many points treated of by him, more or less fully, in some of his lectures delivered in this theatre, and many on which I have had the advantage of his conversation and counsel. His teaching has entered so deeply into, and mingled so closely with, observations and reflections that may have suggested themselves in the course of my work in the museum, that in many cases it would be difficult to trace the sources of information that I may have to impart, and I may, perhaps, appear guilty of appropriating what should belong to another. Let me acknowledge then, once for all, how deeply I am indebted to Professor Huxley, not only for the information conveyed in the public manner of lectures and books, but also for the generous way in which, on all occasions, his time, his knowledge, his thoughts, have been freely given to me. And I say this the more willingly, because I know that I am but one of many whose labours

have been lightened, whose efforts have been stimulated, whose difficulties have been smoothed by his encouragement and support, by his candid and judicious criticism, and by the example of his often self-sacrificing devotion to the advancement of scientific truth.

I also desire, in conclusion, Mr. President, to take this opportunity of publicly expressing the obligations which I, as well as numbers of others, feel towards you and your predecessors in that chair, as well as to the former and present members of the Council of the College, individually and collectively, for the immense aid that you have given to the progress of philosophical biology in this country, and I may say, in the whole world, by the maintenance and augmentation of John Hunter's Museum. An epoch of revolution appears to be at hand in our profession, which may lead to a material alteration in the respective positions and opportunities of the various corporate bodies. Whatever changes may take place, the College of Surgeons will always look back with satisfaction to the fact that, for the first seventy years of the century, when these studies were less appreciated than they are now, or will be hereafter, it has, with scanty aid from the national resources, cherished the growth of a truly national institution, the benefits of which are not confined to any one class or profession, but are freely open to the whole community. I can say,

moreover, with perfect assurance, speaking both from my own experience, and from knowledge of the history of the Museum, as recorded in its archives, that any deficiencies which exist in the condition of the collection, any needs which it does not supply, must be due to other causes than want of encouragement to the officers of the establishment, or want of liberality in supplying the requisite funds, on the part of the Council of the College.

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